

stain from any generalisations, it is obvious that the notion of the rotatory power of saline solutions being independent of the particular metal present in the salt is altogether untenable.

PERCY F. FRANKLAND,

University College, Dundee, March 11.

THE notice referred to by Prof. Percy F. Frankland was written, and the proof returned to the printer, before the end of last year. Since then two researches have been published—by Purdie and Walker, and by Frankland and Appleyard—in which facts are adduced, apparently irreconcilable with Guye's theory. Had these facts been at my disposal I should doubtless have expressed myself more guardedly.

Prof. Frankland says: "As far as I am aware, there is not a single instance of an asymmetric carbon atom attached to four groups *qualitatively distinct*, being found optically inactive in consequence of two of those groups being *quantitatively equal in mass*;" and he complains that I have hardly emphasised this sufficiently. My reason was, that I was not altogether convinced of the fact, as may be seen from the following passage, which I transcribe, and which originally formed a footnote to the notice in question:—

"The present reviewer ventures to suggest that cases such as are sought by Guye are to be found in those compounds in which two of the four different groups attached to an asymmetric carbon atom are themselves asymmetric carbon atoms of equal and opposite enantiomorphism. Such compounds would exist in two distinct forms; but as the two opposite enantiomorphic groups would be of equal mass and would be situated at equal distances from the central asymmetric carbon atom to which they are attached (inasmuch as the two opposite enantiomorphic modifications of a compound always have the same molecular volume), the conditions necessary for optical inactivity according to Guye's theory would be fulfilled, and neither of the two forms ought to cause rotation of the polarised ray. Such a case has already been observed in the two inactive, non-racemic trihydroxyglutaric acids described by Emil Fischer (*Ber. der deutsch. chem. Ges.* 24, p. 4214), although it does not appear to have been hitherto interpreted from this point of view."

I afterwards suppressed this footnote, partly because it seemed to me out of place in such a notice, and partly because the optical activity of the two trihydroxyglutaric acids could be accounted for in another way; namely, by the fact that, as pointed out by E. Fischer, the mirror images of their molecules are congruent with the molecules themselves. But the passage will show why I was indisposed to enter a proved negative against Guye's theory.

As regards the charge of "endorsing special pleading" in the interests of the electrolytic theory of Arrhenius by suppressing the fact that tartar emetic has, in solution, a different rotation from the other metallic tartrates, I may say at once that I was ignorant of this fact. I am not a specialist on the subject of the optical properties of organic compounds, and I merely summarised, doubtless uncritically, the account of Oudemans' law given in van't Hoff's book. Indeed, the brief notice, as its wording everywhere indicated, was a summary rather than a criticism.

I take this opportunity of rectifying an omission. At the time of writing the notice I was not aware that Prof. Crum Brown had, independently of M. Guye, put forward, in the Proceedings of the Royal Society of Edinburgh, views on the influence of the various substituting radicles in modifying the optical rotation of organic compounds.

F. R. JAPP.

University of Aberdeen, March 18.

Standard Barometry.

THE question of absolute accuracy in barometer readings is one of great importance to meteorologists; but there has been so much uncertainty shown by the accumulated facts relating to the subject, that I think that no one who has carefully studied the matter has felt fully satisfied that strictly comparable international standards had been obtained. An uncertainty of at least 0.1 mm. was indicated by the various international comparisons of normal barometers which have been carefully made and discussed during the past ten years. I think that at last a definite conclusion has been reached, and that the very recent results published in paper No. 4, Band xvi. of the *Reperatorium für Meteorologie* will be accepted as proving that at St. Petersburg at least normal readings are obtained.

About twenty-five years ago Director Wild, of the Central Physical Observatory at St. Petersburg, established the first normal barometer of the *modern* form; and as much as twenty years ago he claimed to have obtained practically normal readings. Moreover, he urged that the transfer of these normal readings from place to place by means of portable barometers was impossible within the desired limits of accuracy, and that each country ought to have its own thoroughly investigated normal barometer. This last has been proved by the results obtained by various investigators; and now Prof. Wild offers the proof of the accuracy of his normal barometer in the paper just referred to, which bears the title "Die normal-barometer des Physikalischen Central-Observatoriums zu St. Petersburg."

This paper was presented to the Academy of Sciences on November 4, 1892, and in it Wild gives the results of the inter-comparison of three local normal barometers.

Normal barometer No. I. was mounted at St. Petersburg in 1870, and was fully described in Band iii. of the *Repert. f. Meteor.*

A second normal barometer was mounted at Pawlowsk (about twenty miles from St. Petersburg) in 1887, and a third normal was mounted at St. Petersburg in 1891, and is known as normal No. II.

In 1887 and 1888 Wild found that the St. Petersburg normal I. and the Pawlowsk normal did not differ by more than 0.01 mm.

In 1892 the St. Petersburg normals I. and II. were found to agree within the limit of error of observation (less than 0.01 mm.).

In 1892 the St. Petersburg Normal II. was dismantled, taken to Pawlowsk, and there compared with the Pawlowsk normal, and the two were found to differ by only 0.01 or 0.02 mm.; that is .004 or .008 inch. It must be added that these comparisons have all been checked by means of comparisons with portable barometers of the highest class.

The paper by Prof. Wild is accompanied by illustrations of these various normal barometers. The St. Petersburg normal has recently undergone some alterations, and these are also fully described. Altogether this is perhaps the most important contribution to the subject that has appeared since Prof. Wild's famous memoir of 1873; for we can now rest assured that farther refinement is not required by any practical demands.

It seems to me that now that we are sure of the accuracy of Wild's normal, it is more necessary than ever that we should know with greater certainty its relation to the principal standards of Europe. I desire, therefore, to propose a plan by which a series of comparisons can be carried out for a few places at a very slight expense, and with as much accuracy as portable instruments will permit. In 1883 it became my duty to transport to America, from Hamburg three of the Wild-Fuess portable barometers of the highest grade; and it was of great importance to take every possible precaution against their being injured or their condition altered in any way so as to affect their readings. I devised a mounting on shipboard which was very satisfactory, and gave me no cause for uneasiness regarding the barometers, even in stormy weather. So many barometers are sent out from England to almost every country that I strongly urge the use of a similar arrangement in all cases where it is desirable to retain an assigned barometer correction.

The accompanying sketch shows my manner of mounting the barometers. Two small strips of wood, AA, are screwed to the woodwork running lengthwise of the vessel. They are placed about two feet apart, and are inclined at an angle of perhaps 45°. Small leather straps, say 15 inches long, are fastened to these strips by single screws as shown at BB. A rather soft stuffed flat cushion or pillow is now placed against the woodwork (wall) as shown at C. The box containing the barometer is now pressed against the cushion and the two extremities are placed within the grasp of the straps BB. These last are buckled and drawn tight enough to hold the barometer box firmly against the cushion C. The barometer is thus held in such a manner that no ordinary jarring can cause any damage to it, as there is no direct contact with a rigid surface, since the pillow prevents it from touching the wooden strips, and the soft yielding straps have a spring-like effect.

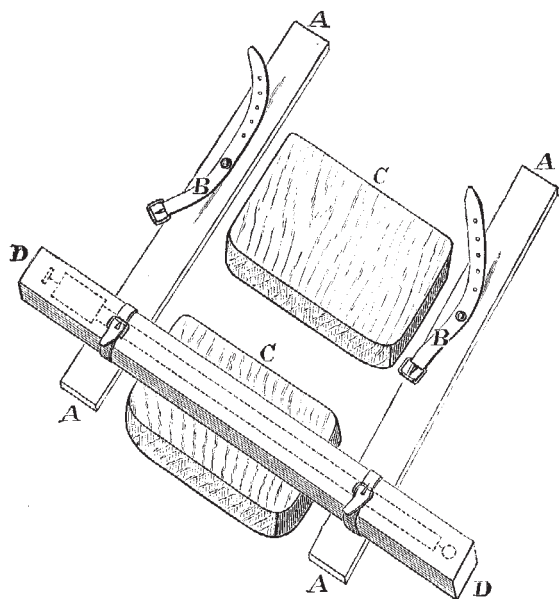
The lower part of the sketch shows the barometer box DD in position, with the barometer shown within it. Of course the cistern is held uppermost. On account of the jarring motion of the ship's screw in rough weather, it is desirable to locate the barometers well amidship, and also have the cistern of the baro-

meter directed towards the stern. Barometers can be placed in this manner on ship board by the maker, and can be left to themselves for any length of time. If the person to whom they are consigned is notified of their subsequent arrival at port, he can take them from their hangings on the ship in the best possible condition. Of course this presupposes an arrangement with the officers of the vessel, such that the instruments shall be let entirely alone from the time they are mounted by the consignor until they are received by the consignee.

I think this method of carrying instruments can be very usefully applied in improving our knowledge of the relation of international barometric standards, and at a minimum expense; and I will give a brief outline of a convenient way for accomplishing it. The Deutsche Seewarte at Hamburg, and the Kew Observatory at Richmond (through the London Meteorological Office), are in the best positions for supervising this work, and I venture to express the hope that the matter will be seriously considered.

I will outline the work when carried on from London.

Let two barometers of the best construction, say an Adie Fortin and a Wild-Fuess control barometer, be compared with the Kew normal during a period of a week or more, or long enough to experience considerable variation in the barometer



height. Then let the two barometers be mounted on one of the London Hamburg steamships, in the manner which I have described, and sent to Hamburg, where an employee of the Deutsche Seewarte could be despatched to take down the instruments and carry them to the Seewarte for comparison with the normal barometer. Then the barometers could be taken by a messenger to Lübeck, at an expense of a few shillings, and mounted on a St. Petersburg steamer, which would carry them almost to the door of the Central Physical Observatory, where they could be again taken in charge by a meteorologist, compared for a few days, and then again be mounted on another steamer bound for one of the Scandinavian ports where there is a standard barometer, and finally returned to London by one of the numerous regular steamships. At an expense of a couple of pounds the barometer could be sent from St. Petersburg (or Scandinavia) back to Hamburg *via* Stettin and Berlin; thus allowing Berlin to enter into the series. The barometers would probably have to be sent by a messenger from Berlin to Hamburg, thus entailing the just mentioned expense. A second comparison at Hamburg would be desirable, and then the barometers could be returned to London by sea, and again compared at Kew.

Similarly, barometers could be sent to New York for comparison with the sub-standard, by Adie, at the Maritime Exchange; although probably the United States Weather Bureau would assume the expense of the three pounds necessary to carry the

instruments to Washington for comparison with the normal there, and then return them to New York and put them on ship-board to be returned to London.

The standard barometers of Australia, India, Brazil, and other countries accessible by sea can be reached from London (or Hamburg) in the same way, and the comparison instruments can be returned to their starting point for additional verification.

My own experience in the transportation of barometers assures me that ship captains would gladly give their hearty co-operation to a work of this kind, and there would be no charges for carrying the instruments even half round the world and back again.

In offering this suggestion it is not necessary for me to give the details for the complete organisation of such a scheme; but it may be remarked that if it should be undertaken, the personal experience of those who have been over the ground should be utilised in making plans. A single instance will serve to show why this is advisable. Some years ago I carried two barometers from Hamburg to London by sea. I took the German line of steamers and found myself anchored in the middle of the Thames, and had to get ashore as best I could. I greatly feared that I should never get the barometers ashore in a whole condition, as there was necessitated a great deal of scrambling over lighters, &c., and embarkation in an unsteady row boat in order to make a landing. Had I taken the English steamer, all this worry would have been saved. Other similar instances occurred which could have been avoided by one personally familiar with the routes to be travelled.

FRANK WALDO.

Princeton, New Jersey, February 20.

Motion of a Solid Body in a Viscous Liquid.

THERE is perhaps no branch of mathematical physics which has made greater progress during the last thirty-five years than hydrodynamics. During this period numerous important investigations have been published upon the motion of solid bodies in a *frictionless* liquid, upon the theory of discontinuous motion, upon the theory of vortex motion and vortex rings, upon the motion of a liquid ellipsoid under the influence of its own attraction, and upon waves and tides. These investigations constitute an enormous increase in the knowledge possessed by the present generation compared with that of its predecessors; they have to a considerable extent exhausted the field of research in the theory of the motion of *frictionless* liquids; but notwithstanding the importance of the results, the elegance of the methods by which many of them have been obtained, and the skill by which the mathematical difficulties have been surmounted, all the investigations referred to possess the defect of not accurately representing the motion of liquids as they occur in nature.

The reason of this discrepancy between theory and observation is that the ideal substance, which is called a *frictionless* liquid, has no actual existence, for all liquids which occur in nature are *viscous*. The viscosity of the mobile liquids, such as water, alcohol, &c., is a small quantity, being in the case of water equal to a tangential stress of about $\cdot 014$ dynes per square centimetre; whilst in the case of the sticky and greasy liquids, such as treacle and oil, it is much greater. The viscosity of olive oil is about $3\cdot 25$ dynes per square centimetre, and is therefore about 232 times as great as that of water.

The mathematical theory of the motion of viscous liquids was elaborated as long ago as 1845 by Sir G. Stokes, in a paper in which he showed that the effect of viscosity might be represented by certain additional terms in the equations of motion of a *frictionless* liquid, which contain as a factor a new physical quantity called the viscosity. In a subsequent paper, published in 1850, he applied the above theory to calculate the diminution of the amplitude of the small oscillations of a sphere surrounded by water; and by means of experiments in which this quantity was observed, he calculated the numerical value of the viscosity of water, and found that it was in close agreement with the value found by Poiseuille from experiments on the flow of liquids through capillary tubes. An investigation of a similar character was undertaken by von Helmholtz and Piotrowski about 1863, in which the sphere was suspended by a torsion fibre, and made to perform small torsional oscillations about a diameter.

Almost all calculations relating to small oscillations proceed upon the basis that the squares and products of quantities, upon which the disturbed motion depends, may be neglected. This introduces a great simplification into the work, and enables a variety of problems, which would otherwise be exceedingly intractable,